tion is made at the capricious decision of a body of resident conservatives who, through the Senate, wield a wholly disproportionate power on matters vitally affecting the well-being of the University." Yet he had just admitted that only thrice in twenty-five years has the Senate come up in force, and that only once did it outvote the resident majority. His picture of wicked conservatives "constantly" calling up non-residents to oppose progressive Radicals is just as devoid of fact as the assumption that Radicals are always progressive.

When, in 1910, a like charge was made against the Senate, I recited in the Senate-House a list of much-needed reforms and progressive measures (in all of which I was concerned), *e.g.* a proper audit and control of Departmental funds, the reform of the Press, the reform of the Fitzwilliam Museum, the founding of the Departments of Anthropology and of Architecture, etc., etc. (all of which had later to be carried out), and I charged to their faces the Radical leaders who then controlled the Council, with heading the obstruction to all these reforms. No one then or since has disputed my allegations. The suggestion that resident conservatives are an insignificant body is disproved by our important gains in the late elections to the Council (even without the much-needed secret ballot). My critic does not deny that the proposals of the Report respecting the powers of the Senate, the constitution of the Council and of the Board of Studies and Research go much further than the proposals made by the committee of younger graduates (men under 40) who represent the most advanced opinion among residents. They wish that the ultimate decision on statutes should rest with the Senate, and that the professoriate should keep its representation on the Council, and have some on the proposed new Board of Studies.

WILLIAM RIDGEWAY.

Flendyshe, Fen Ditton, Cambridge,
December 2.

LEAVING on one side the more personal aspects of Sir William Ridgeway's letter—his zeal for progressive reforms and the disingenuousness of his critic—a reply may be made to one or two of the points raised by him. He is mistaken in saying that the committee of younger graduates (men under 40) represent the most advanced opinion among residents. They have not unfairly been nicknamed "The Cambridge Whigs." Even this body, however, has suggested that members of the General Board (or Board of Studies and Research) should be nominated by the Council and that the Board should be reduced in size by abandoning the direct representation of the Special Boards of Study. It is held by many, who are equally keen with Sir William Ridgeway on the independent development of educational policy in the University, that the best solution lies in a small Board akin to the present Board of Research Studies, well balanced between the different faculties and working in close co-operation with the Special Boards. As to the question whether an appeal against the House of Residents would ever be made under the Commission's scheme, the writer differs absolutely from Sir William Ridgeway. Differences of opinion are too acute, and the fighting spirit of both sides too strong, to allow certain proposals to pass without a stern contest at every possible point.

THE WRITER OF THE ARTICLE.

Gravity Variations.

MR. R. D. OLDHAM's letter in NATURE of November 18, p. 665, makes the disquieting suggestion that the force of gravity at Dehra Dun may be subject to fluctuations. The changes that he points out in the times of oscillation of the Indian pendulums can, however, be quite reasonably attributed to alterations in the lengths of the pendulums and errors of observation, and are not, in my opinion, so grave as to warrant a belief in anything more fundamental. As Mr. Oldham says, there is neither proof nor disproof of a change in gravity. But the discussion undoubtedly indicates a weak spot in the Indian operations, namely the connexion of Dehra Dun with Kew Observatory, which is the base station for this country. It rests on the results obtained with four pendulums swung at Kew and then transported to Dehra Dun and swung there. The pendulums have never been brought back to this country, so if they suffered any changes of length on the journey from Kew to Dehra Dun the value of g found at the latter place will be erroneous.

It would, of course, have been necessary to undertake a return journey long ago if no corroboration of the result of the first journey had been available. There was, however, the strong corroboration afforded by Hecker's observations in 1905, as mentioned by Mr. Oldham, and the valuable though less powerful evidence obtained by Alessio in 1906. Hecker's result was of special value because at Jalpaiguri his apparatus was set up alongside the Indian one and simultaneous observations were made using the same clock. Thus there was good reason to believe that the effects of fluctuations of temperature and variations in the clock's rate—the chief sources of uncertainty—would be the same on both sets of observations, and that therefore the check on the Dehra Dun value of g would be nearly as satisfactory as if Dr. Hecker's pendulums had been swung at Dehra Dun itself.

The links forming the connexion of Dehra Dun with the value of g determined at Potsdam are as follows:

	Result.
Potsdam—Kew (Putnam, 1900)	
Kew—Dehra Dun (Indian operations, 1904)	979·063
Potsdam—Jalpaiguri (Hecker, 1905)	
Jalpaiguri—Dehra Dun (Indian, 1905) .	979·065
Potsdam—Genoa	
Genoa—Bombay (Alessio, 1906)	
Bombay—Dehra Dun (Indian, 1904) .	979·059

The probable error of each of these results may be estimated to be between ±0·003 and ±0·005. The agreement between them is therefore better than the probable errors would have allowed us to anticipate.

Commander Alessio's observations in 1913–14, however, give a value of 979·079 for g at Dehra Dun, which differs from the above by nearly four times the probable error.

Alessio's observations were most carefully made with a strong equipment of eight pendulums, and carry great weight. They have not, so far as I am aware, been published in detail as yet, and it is not possible to form a final judgment on them; but in the article in the *Rivista Marittima* quoted by Mr. Oldham, there is a remark which may perhaps indicate a weak point. Commander Alessio says that the comparison of the times of oscillation of the pendulums at Genoa before and after the journey show that certain changes had taken place in the lengths of the individual pendulums, but that fortunately the length of the mean of the eight pendulums had remained absolutely unchanged. If the changes in the individual pendulums were large, and if they, or any of them, took place before the pendulums reached Dehra Dun, then the deduced value of g at Dehra Dun may be burdened with a considerable error.

Whatever opinion may be formed when the whole of the details of Commander Alessio's work are avail-

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able for examination, it is clear that the Dehra Dun value of *g* should be strengthened by a new direct determination of the difference Kew—Dehra Dun.

This could be made by sending the Indian pendulums back to Kew for a further set of observations to be made there, or, if the use of Invar pendulums is contemplated, then the new set of pendulums could be employed for this purpose. It is imperative that the value of *g* at Dehra Dun should be established so thoroughly as to be unimpeachable.

G. P. LENOX-CONYNGHAM.

Trinity College, Cambridge,
November 29.

THE remarks by Mr. Oldham in NATURE of November 18, p. 665, relating to a suggested variation in gravity, are of great interest. As a result of measurements of *g* at Melbourne in 1913, a doubt as to the invariability of *g* relative to that at Potsdam was forcibly borne to mind. The report (Gravity Observations, British Antarctic Expedition, 1910–1913) which gives the results of the Melbourne measurements, has been delayed in the press, but it is felt that there is some evidence in this case of a lack of constancy in the value of *g* relative to Potsdam.

The problem is discussed in greater detail from another point of view in the Glaciological Report (Wright and Priestley), which is due to appear shortly.

C. S. WRIGHT.

Wey Lodge, Portmore Park, Weybridge,
November 20.

Action of Cutting Tools.

IN the interesting letters by Mr. Mallock and Prof. Coker which have recently appeared in NATURE, some points of importance to the elucidation of the action of a tool when operating on materials have been raised.

Mr. Mallock appears to adhere to the view expressed in his paper of 1881 that the action is simply a phenomenon of shear. H. Tresca, however, two years after Mr. Mallock's paper showed in his classical and extensive "Mémoire sur le rabotage des métaux" (*Mémoires présentés par divers savants à l'Académie des Sciences de l'Institut de France.* Tome 27, No. 1, 1883) that the phenomenon was primarily one of plastic flow. The periodic rupture of the chip which takes place is subsequent to the plastic flow stage and depends upon the nature of the material being operated upon, the angle the tool face presents to the advancing stream of material, and the velocity with which the material moves relative to the tool.

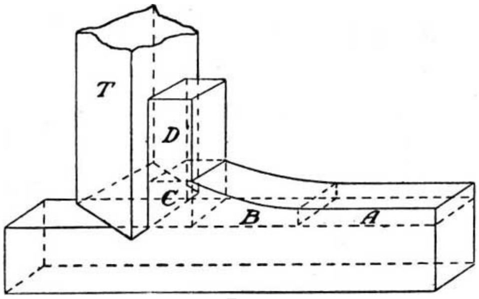

FIG. 1.

This stage of the action is complex and does not appear to be understood fully. The plastic flow stage, however, is comparatively simple.

In the diagram (Fig. 1) suppose that the tool T presents a plane face square to the advancing material. The portion A, which will ultimately form the chip D, as it approaches the tool begins to flow in region B, which is Tresca's *zone d'activité.* The flow reaches a maximum in the region C from which the chip or jet of metal D emerges, and Tresca in the light of the results of his remarkable and historical investigations on the flow and deformation of solids likens the action to the flow of the metal through a tube of shape ABC with its orifice open horizontally at the top part of C. Since no change in the density takes place the product of the co-ordinates xy (where the origin is at the tool edge) of a point on any surface in B and C continuous with a horizontal plane in A must be constant, so that the traces of these surfaces in the sides and also the free edges of B are hyperbolas.

This zone B can be seen in some of the beautiful photographs of cutting tools published by Mr. J. F. Brooks (Proc. Inst. Mech. Engrs., 1905, p. 365) and more especially in the last photograph of Plate 10. If now vertical lines be scribed upon the sides, the state of affairs during flow of a material which does not rupture for large body-shifts, such as lead, is represented by Tresca in Fig. 2.

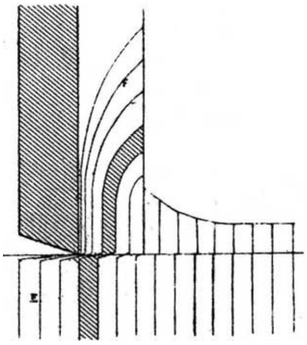

FIG. 2.

Here the maximum slide velocity is at the edge of the tool and in the horizontal plane through the edge. But one of the two important principles enunciated by Tresca is that during flow the maximum shear and maximum slide velocity are co-directional. We should therefore expect the material to rupture along this horizontal plane, and I think this can clearly be seen in Plate 11 of Brooks's photographs of the tool in action on mild steel.

Turning now to Prof. J. T. Nicolson's and Dempster Smith's experiments (*Engineer*, 1905, p. 358) and their diagram of the formation of a chip (Fig. 9), it may be seen that though the diagram is complicated by rupture phenomena and by the fact that the tool is acting on a wedge-shaped part of the forging, Tresca's representation of the plastic phenomena is well substantiated and the maximum shear is clearly seen in the initial stages.

The start of rupture along the horizontal plane is also clearly shown by Frederick Taylor in his presidential address before the American Society of Mechanical Engineers in 1906 (vol. 28), which is a monumental work on "The Art of Cutting Metals."

The same views are expressed by C. Codron in his extensive series of "Expériences sur le travail des machines-outils pour les métaux," published in the "*Bulletin de la Société d'Encouragement pour l'Industrie Nationale,*" 1903–1905.

The second important principle enunciated by Tresca, namely, the maximum shear across any face of a small right six face is a constant = K (Tresca's plastic modulus), together with the one already mentioned, enabled Saint Venant to develop the general equations of plastico-dynamics. If the mathematicians

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